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XVIII. *An Investigation of the lateral Explosion, and of the Electricity communicated to the electrical Circuit, in a Discharge: By* Joseph Priestley, *LL. D. F. R. S.*

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SEVERAL years before I made any experiments in electricity, except with a view to amuse myself, and my friends; I had observed, that in discharging jars, and particularly such as were filled with water, without any coating on the out side, I felt a slight shock; though it was plain that the hand, in which I held the discharging rod, made no part of the circuit.

Mr. Wilson, also, in his first experiments on the Leyden phial, observed, that bodies placed without the electric circuit would be affected with the shock, if they were only in contact with any part of it, or very near it. Analogous to this was his observation, that, if the circuit was not made of metals, or other very good conductors; the person who laid hold of them, in order to perform the experiment, felt a considerable shock, in that arm which was in contact with the circuit. See History of Electricity, p. 95.

Lastly, in the course of my experiments with large electrical batteries, I found the force of this

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lateral explosion (as I shall chuse to call it) to be very considerable: for I several times observed, that a chain which communicated with the outside of the battery, but which made no part of the circuit, made a black stain on a piece of white paper, on which it accidentally lay, almost as deep as the chain that formed the circuit. (History, p. 644.) And when, in order to judge, by my feeling, of the lateral force of electrical explosions; I made it pass over a part of my naked arm, the hairs of the skin were all singed, and the *papillæ pyramidales* raised, not only along the path of the explosion, but also wherever any part of the chain had touched it, though not in the circuit. Ib. p. 686.

It was to ascertain the nature and effects of this lateral explosion, that the following experiments were made; and in reciting them, I shall distinctly note the progress of my own thoughts in the course of the investigation, from a state of absolute uncertainty, to that of the fullest satisfaction; and, I flatter myself, that some of the facts I shall exhibit, will give surprize and pleasure to those, who are best acquainted with, and most interested in, the history of electricity.

Not having the least doubt, but that if any electric spark passed between a body that was insulated, and another, the insulated body would appear, either to have received, or to have lost, electricity; I imagined that nothing more was to be done, than to insulate bodies placed within the influence of the electric circuit, with pith balls hanging from them; and, upon their diverging with the electric spark, immediately to observe, of what kind the electricity

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they had contracted was; and previous to the experiment, I conjectured it would be negative; ſuppoſing that the diſcharge from the inſide coating, in an interrupted circuit, was not able to ſupply the outſide faſt enough. And ſince, the larger the inſulated body was, the greater quantity of the electric fluid it was capable of receiving, or parting with, and conſequently the more ſenſible the effort would be; I began with ſuſpending on ſilken ſtrings, a paſteboard tube, covered with tinfoil, ſeven feet long and four inches thick, with large knots at each end; and a braſs ball (at the end of an iron rod, which communicated with the outſide of the jar) was placed within about a quarter of an inch of it; while the diſcharge was made through an inſulated interrupted circuit, no part of which was leſs than two feet from the inſulated tube. On making the exploſion, the ſpark appeared as I expected; but, to my great ſurprize, I could not find that either poſitive or negative electricity was communicated to the inſulated tube. Neither the pith balls, nor the fineſt threads diverged, or moved in the leaſt, at or after the diſcharge; though, every thing elſe remaining in the ſame ſtate, the leaſt ſenſible electricity communicated to this tube (a quantity ſo ſmall as hardly to be viſible, in the form of a ſpark, at the time of communication) made the balls and the threads ſeparate to a great diſtance, and would have kept them in a ſtate of divergency more than an hour. Leſt a ſmall degree of motion or divergency ſhould eſcape my notice, while I was intent upon making the diſcharge, I had an aſſiſtant along with me, whoſe eye

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was upon the threads all the time I was making the experiment.

This experiment, as will eaſily be imagined, ſhook my whole hypotheſis, and confounded all my ideas. I could not queſtion the fact, having repeated the experiment, with preciſely the ſame event, I believe, above fifty times, on account of having been hardly able to believe my own ſenſes, or thoſe of others. There was an evident electric ſpark, ſometimes near half an inch in length, betwixt the bodies compoſing the electric circuit and the inſulated tube, in ſuch a ſtate of the air, as I knew, by frequent trials, would have kept it electrified a long time, and yet there was no communication of electricity.

I do not remember that I was ever more puzzled with any appearance in nature than I was with this; and, in the night following theſe experiments, endleſs were the ſchemes that occurred to me, of accounting for them, and the methods with which I propoſed to diverſify them the next morning, in order to find out the cauſe of this ſtrange phænomenon. Accordingly, I was no ſooner at liberty to attend to this experiment; but, repeating it with ſome difference in the diſpoſition of the apparatus, I obſerved that, upon every diſcharge, a ſlight motion was given to the threads that hung from the inſulated tube. Upon this the impoſſibility of an electric ſpark, neither giving nor taking any thing from an inſulated body, contrary to my moſt attentive obſervation, and that of my aſſiſtants, I concluded that ſome motion muſt have been given to the threads the day before; eſpecially when I found that, in theſe latter experiments,

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the communicated electricity was always positive, the same with that of the inside of the jar; but the quantity of it was so small, that the most exquisite contrivance was necessary to ascertain the nature of it; for though, upon this occasion, the lateral spark was near a quarter of an inch in length, the threads on the insulated tube could only be made, by the explosion, to change their position, from leaning a little one way, to leaning as much the other, in the neighbourhood of an insulated brass rod, loaded with a small quantity of positive or negative electricity.

I could not help, however, being surprized, that so large a spark should give no more electricity to the insulated tube, than it appeared to have done; when, in other circumstances, a spark ten times less than this would have made a great and permanent alteration: yet, improbable as these circumstances were, I entertained no doubt at that time, but that these insulated bodies were electrified, either positively or negatively, according as the inside of the jar was positive or negative, by this lateral explosion, though the degree was exceeding small; and I continued in this persuasion the longer, as it happened to be a considerable time before I got another spark that communicated no sensible electricity. I cannot help taking notice, that if it had happened, that, in my first experiment, the insulated tube had always acquired or lost the least sensible electricity (and, as I afterwards found, there were many chances against the first result) I should have formed, and have acquiesced in, some sort of hypothesis, to account for the giving or receiving of electricity in those circumstances,

ſtances, and there the buſineſs would have ended; but the ſeeming contrariety of theſe appearances obliged me to purſue them farther.

Not being able completely to ſatisfy myſelf with my laſt concluſion, attended with the difficulties above mentioned, I kept diverſifying the experiments, and introducing every circumſtance that I could imagine might poſſibly affect the reſult of them; and among the reſt, I made the following experiments, which quite unhinged me again, and left me as much at a loſs as ever I had been before.

Having ſuſpended a fine thread on an inſulated braſs rod, placed about $\frac{1}{8}$ of an inch from another rod, which was likewiſe inſulated, and one end of which was in contact with the coating of the jar; and having electrified the rod that ſupported the pith balls, and placed a rod loaded with the ſame electricity near them: I obſerved that, upon every diſcharge, the balls, which before were repelled, were inſtantly attracted by the electrified rod; and, that the reſult was invariably the ſame, whether they and the rod were loaded with poſitive or negative electricity; and alſo whether the jar was charged poſitively or negatively. I repeated the experiment for ſeveral hours, without the leaſt variation in the event; which clearly proved, that, in theſe circumſtances, the electricity of the rod that received the lateral exploſion was diſcharged by it.

Afterwards, I repeated this experiment with ſome little variety, and found the electricity of the rod only leſſened by the lateral exploſion. Theſe experiments, however, by no means favoured the ſuppoſi-

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tion of the uniform communication of electricity, either that of the inside or that of the outside of the jar; and, together with the former experiments, convinced me, that this lateral spark by no means produced the effect that might have been expected in communicating electricity. But, with the next set of experiments, the difficulty began a little to clear up, and continued to do so gradually, till I gained all the satisfaction I could wish for, with respect to this puzzling phænomenon.

The first time that I was able to vary the electricity of the insulated body placed near the electric circuit, or of the bodies that formed the circuit (which I now began to attend to), by any different adjustment of the apparatus, was on the following occasion.

Near to an iron rod, that touched the bottom of a jar charged positively, I placed another insulated rod, with a pair of pith balls hanging to it; and observed, that, when I attempted to make the discharge, through an imperfectly conducting circuit, (bringing *e*, *g*, part of the table into it), a strong spark passed between the insulated rod and the other that touched the jar, and immediately the balls separated as far as they possibly could; and, continuing in a repulsive state, appeared to be electrified negatively. But immediately completing the circuit with good conductors, and making the remainder of the explosion in a full spark; another spark passed between the two rods, and immediately the balls fell close together again; and sometimes would separate with the opposite, *i. e.* positive, electricity.

I could not, upon this occasion, make the lateral spark, in the full explosion, so great as in the im-

perfect discharge. I also observed, that the more interrupted the circuit was, the farther would the lateral explosion reach; and that the electricity, which the full explosion communicated, was always positive when the jar was charged positively, and negative when it was charged negatively. The result of the imperfect discharge was always the reverse.

Insulating several bodies, and the jar too, charged positively, they all equally contracted positive electricity by the discharge.

In this state of the experiments, I had no idea of the possibility of the lateral spark not communicating electricity to the insulated body; but I imagined that the kind of electricity communicated depended upon some circumstance in the disposition of the apparatus, that I was not sufficiently aware of.

At length recollecting, that this last experiment resembled, in some respects, that curious one of professor Richman, mentioned in the History of Electricity, p. 272, in which it appeared, that when the coating of either side of a plate of glass communicated with the ground, the opposite electricity of the other side was more vigorous; I was satisfied that the negative electricity of the bodies that formed the circuit in the imperfect discharge, was produced by the greater difficulty with which the outside of the jar was supplied, than the inside was discharged; so that the outside was comparatively in a state of insulation, and therefore would communicate negative electricity to all bodies within its reach. And from this I was led to conclude, that, provided the jar was insulated, and the inside

was

was made to part with its electricity with more difficulty than the outside received it, the bodies that formed the circuit would contract positive electricity; and the result answered exactly my expectations.

I also concluded, that, making the interruption in the middle of the circuit, since, in this case, the inside would give, and the outside receive, with equal difficulty, the bodies in the circuit, placed between the place of interruption and the inside of the jar, would be charged positively; and those placed between the place of interruption and the outside, would be charged negatively; and this also was verified by experiment.

In this state of things, I found, that I could give the insulated circuit what kind of electricity I pleased, provided there was any kind of interruption in some part of the circuit; and conjecturing that the electricity of bodies placed near the circuit would be the same with that of the bodies that composed it, I sometimes placed the rod that supported the pith balls near the circuit, and sometimes introduced it into the circuit; and found, that, in both cases, it contracted the same electricity. This tended to confirm me in my supposition, that the lateral explosion was always attended with a giving or receiving of electricity, according to the nature of the circuit, and the place where it was situated; and I again overlooked the disproportion between the cause and the effect.

Presently after this, it occurred to me, that what may be called the redundant electricity of the outside or inside of the jar, separates from that which is in the glass, and constitutes the charge, must have some

ſome concern in this event; and the ſuppoſition was verified by fact. For, inſulating a jar, charged poſitively, I obſerved, that when I touched the outſide coating laſt (as is commonly done in ſetting it down) and made the diſcharge through good conductors, they were all electrified poſitively; and bodies placed near the circuit were the ſame. On the contrary, if, after placing the jar upon the ſtand, I touched the knob of the wire, communicating with the inſide, ſo as to take off all its redundant electricity; both the circuit and the neighbouring bodies contracted negative electricity.

I had at this time quite forgot that Æpinus had made the ſame obſervation, on diſcharging a plate of air, mentioned in the Hiſtory of Electricity, p. 273; but, conſidering what he ſays on that ſubject, I find he was miſtaken with reſpect to the reaſon of this experiment not ſucceeding with Dr. Franklin and others, who had always aſſerted, that the electric circuit contracts no electricity at all by a diſcharge. For he ſays, that the ſurfaces with which the doctor tried the experiment, were not large enough to make the effect ſenſible; and that the diſtance of the metal plates was likewiſe too ſmall, as, he ſays, it neceſſarily muſt be in the charging of glaſs: whereas I could give the inſulated circuit as ſenſible electricity with a common jar, as he could with his plate of air; and much more depends upon the height of the charge, which muſt have been inconſiderable in the plate of air, than the quantity of ſurface; which, however, may be increaſed at pleaſure, by multiplying jars in batteries.

 I found,

I found, however, afterwards, that much depended upon the quantity of ſurface in the coating, and the bodies connected with them, as containing more of that redundant electricity, the effect of which was ſeen in the laſt mentioned experiment. For when I diſcharged the jar, ſtanding in contact with the prime conductor, the tendency to the communication of poſitive electricity was ſo great, that, in that ſituation, the inſulated circuit contracted ſtrong poſitive electricity, when, every thing elſe remaining the ſame, except removing it from the conductor, and then making the diſcharge, it contracted no electricity at all.

Being now perfectly maſter of the electricity of the circuit in electrical exploſions, and being able, in two different methods, to give which of the two electricities I pleaſed; I imagined that, if I could ſo balance them, as to communicate neither, there would be no lateral ſpark, as in the abovementioned experiments; but in this I was abſolutely miſtaken. For,

In the firſt place, when, after ſetting the charged jar upon the ſtand, I took off, as near as I could gueſs, one half of the redundant electricity of the inſide, and left both ſides equally electrified (as appeared by the equal attraction of the pith balls to them both), the diſcharge of the jar through a circuit of good conductors did not, indeed, communicate the leaſt ſenſible electricity to the circuit, but the lateral exploſion was almoſt as manifeſt as before. The pith balls, hung upon the rod that received it, never ſeparated.

In the next place, I repeated this experiment by balancing the two different methods of communicating electricity to the circuit, one against the other. For, not insulating the jar, but setting it on the table, which gave the circuit and the bodies contiguous to it an advantage for contracting positive electricity by the discharge; but, at the same time, making an interruption in the circuit (by introducing part of the table into it, which tended to give them negative electricity); I could easily manage it so, that the circuit contracted neither the one nor the other; and yet, as in the former case, the lateral explosion was as considerable as ever. The balls never separated.

To vary the experiment, I placed an insulated brass ball, two inches in diameter, round and smooth, so as not easily to part with any electricity it had got, in the place of the rod that supported the pith balls; and having found a situation in which no electricity was communicated to the circuit, I observed that none was communicated to it, though, to all appearance, it received a spark of about $\frac{1}{4}$ of an inch in length. At least, if it had contracted any, it was so little, as to make it very problematical; whether a pith ball, or a fine thread, was moved by it, or not: whereas, when I gave it the smallest sensible spark in any other manner, it would attract those light bodies for a long time together.

The interruption of the circuit I made use of in this experiment, was not by means of any part of the table, but only about a yard of brass chain introduced into it, and disposed between the inside of the jar and that part of the circuit, near which the

inſulated ball was placed. N. B. The ball muſt not be placed near the jar itſelf; for, in that ſituation, I found, that, though it was very ſmooth, and perfectly ſpherical, yet it could not be placed very near the outſide of the jar ſtanding on the table, without contracting negative electricity, in a very ſmall ſpace of time.

Theſe experiments threw me back into my former ſtate of perplexity, with reſpect to the lateral ſpark; ſince, when the two electricities of the circuit were exactly balanced, it was very little diminiſhed, and yet the body that received it was not in the leaſt ſenſibly electrified. But, upon reflection, I concluded, that this lateral ſpark muſt be of the nature of an exploſion, and conſequently, that an electric ſpark muſt enter, and paſs out again, within ſo ſhort a ſpace of time, as not to be diſtinguiſhed, and leave no ſenſible effect whatever: for though, in this caſe, part of the electric matter natural to the body muſt be repelled, to make room for the foreign electricity, its reſtoration to its natural ſtate was ſo quick, that no other motion could correſpond to it.

This hypotheſis is favoured by the obſervation, that it is the very ſame thing whether a body be introduced into a circuit, or placed near it, with reſpect to contracting electricity; that is, whether the electric charge enter the body at one place, and go out at another, or whether it be received and emitted at the ſame place.

This lateral exploſion is an effect ſimilar to a partial circuit, in which, part of the electric matter that forms the charge in an exploſion, goes one way, while the reſt of the charge goes another; the only

difference

difference is, that this detached part of the charge leaves the common track, and returns to it again, in the very ſame place.

Several remarkable partial circuits occurred in the courſe of my experiments before, particularly one, mentioned in the Hiſtory of Electricity, p. 692, in which, part only of the exploſion paſſed in the ſhorteſt way, while another part of it took a circuit, conſiſting of the ſame materials, thirty times as long; and another, mentioned, p. 691, where one circuit was made through a thick rod of metal, and another, at the ſame time, through the open air.

That there is an admiſſion and an exploſion of the electric matter, in this lateral exploſion, ſeems evident, from this circumſtance, that it is far more conſiderable when the body that receives it is large, than when it is ſmall. In the former caſe, there is room for the electric matter, natural to the body, to retire, upon the admiſſion of the foreign electricity belonging to the charge; whereas, in the latter caſe, there is not room for it. When I placed a ſmall braſs ball, of about a quarter of an inch in diameter, near the circuit, I could not perceive that it was at all affected by any lateral exploſion; and the ſpark was very inconſiderable, when I placed a needle, about two inches in length, to receive it; but when I connected the large tube above mentioned, by means of a pretty thick iron wire, to any body whatever, that was placed in the neighbourhood of the circuit, I have (with a jar of only half a ſquare foot of coating glaſs) made the lateral exploſion, an inch or more in length, conſiſting of a very full and bright ſpark of electric fire. Inſulated bodies, of about

about eight or nine feet in length, ſeem to admit as large a lateral exploſion as any body whatever is capable of: for, connecting them with the ground, by means of the beſt coductors (which gave the electric matter in the bodies, the freeſt receſs poſſible) I could never make this exploſion much more conſiderable, uſing the ſame jar, and all other circumſtances the ſame.

It is a manifeſt advantage in theſe experiments, that the lateral exploſion be not taken from the coating of the jar itſelf, or from any part of the circuit, very near to it. I have found that, *cæteris paribus*, it is the moſt conſiderable taken at the extremity of a braſs rod, of one foot, or a foot and a half long, the other end of which is contiguous to the jar. It is analogous to this, that the longeſt ſpark is taken, not from the body of the prime conductor itſelf, but at the extremity of a long rod inſerted into it. The electric matter ſeems to acquire a kind of *impetus* by the length of the medium, through which it paſſes. But I found that the *maximum*, in this caſe, did not exceed, or rather, that it did not quite reach, three feet; for, making uſe of a thick iron rod, eight or nine feet long, the lateral exploſion, taken at the extremity of it, was about the ſame, as when it was taken at the end of a rod four inches from the jar; and not half ſo conſiderable as when taken at the extremity of a rod one foot long. This, I imagined, might be owing to the obſtruction which the electric fluid meets with in paſſing even through metals; which appears, by my former experiments, to be much more conſiderable than was generally imagined.

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Upon the whole, this remarkable experiment ſeems to be made to the moſt advantage in the following circumſtances. Let the jar ſtand upon the table; let a thick braſs rod, inſulated, ſtand contiguous to the coating; and, near the extremity of this rod, place the body that is to receive the exploſion. This body muſt be ſix or ſeven feet in length, and, perhaps, ſome inches in thickneſs, or be connected with a body of thoſe dimenſions. Laſtly, let the exploſion be made with the diſcharging rod reſting upon the table, cloſe to a chain, the extremity of which reaches within about an inch and a half of the coating of the jar. In this caſe, the operator will hardly fail of getting a lateral exploſion of an inch in length; which ſhall enter and leave the inſulated body, without making any ſenſible alteration in the electricity natural to it.

With large jars, containing three or four ſquare feet of coated glaſs, bearing a very high charge, I make no doubt but that this experiment might be made to much more advantage; but, at the time that I was engaged in this inveſtigation, I happened not to have any ſuch jar, and therefore only uſed one that contained half a ſquare foot of coated glaſs.

If the interruption in the circuit, which is almoſt neceſſary in theſe experiments, be made by introducing a length of chain into it, rather than by making part of the exploſion paſs along the tube, there is a medium in the length of chain, that anſwers better than either a longer or a ſhorter circuit. In a long interrupted circuit, the electric matter ſeems

to lofe the *impetus* which it difcovers in a fhort one.

In all thefe cafes, the electric charge feems to remain for a moment in the parts of the interrupted circuit; and therefore inftantaneoufly rufhes, in all directions, as well towards bodies that are not placed along its paffage to the jar, as thofe that are; but, when the fame charge occupies a larger circuit, it has more room to expand itfelf, and is not fo ftrongly impelled to defert it. I found, however, by repeated trials, that when I made ufe of three yards of brafs chain in the circuit, there was a diftance to which the lateral explofion would not reach. The fame diftance it alfo would not reach, when the circuit confifted of only one brafs rod; but it reached it with great eafe, when only half a yard of chain was ufed, even without any other interruption in the circuit. But it reached to a much greater diftance, when the chain was very fhort, and the interruption was greater in other refpects.

I had imagined, that, fince the body which had received the lateral explofion contained, for a moment, more than its natural quantity; that, if it were acutely pointed, fome would efcape, and that, upon the return of the explofion, the body would be exhaufted; but I found no fuch effect, though I affixed fine needles to the bodies I made ufe of. The lighteft pith balls, placed near the extremities of thefe needles, were not in the leaft affected by the explofion.

When I placed a number of brafs balls, one behind another, the lateral explofion paffed through them all, being vifible in the intervals between each

of them, and returned the ſame way, leaving them all in the ſame ſtate in which it found them; and a great number of lateral exploſions might be taken at the ſame time, in different parts of the circuit, ſome of them very near one another.

It made no difference, whether the lateral exploſion was received on a flat ſmooth ſurface, or the points of fine needles. In both caſes, the ſpark was equally long and vivid.

I had no ſooner completed theſe experiments on the lateral exploſion; but I had a curioſity to ſee what kind of an appearance it would make *in vacuo*; ſince no other phænomenon in electricity reſembles it. In all other caſes, the electric matter ruſhes in one ſingle direction; whereas, in this, it goes and returns in the ſame path, and, as far as can be diſtinguiſhed, at the ſame inſtant of time; ſo that all the difference of the two electricities, which are ſo conſpicuous *in vacuo*, muſt here be confounded. Accordingly, I found, though my pump was not in good order, that I could perceive this exploſion *in vacuo*, at the ends of rods, placed ſeveral inches aſunder; and when they were brought within about two inches, they ſeemed to be joined by a thin blue or purple light, quite uniform in its appearance. As theſe rods were made to approach, this light grew denſer; but ſtill exhibited no ſuch variety, as is obſerved between the bodies that give and receive electricity, in the common experiments *in vacuo*.

I was pretty ſoon convinced, that uncoated jars could not be uſed to any more advantage in theſe experiments, than thoſe that were coated; ſince the want of coating only operated as an interruption in

the circuit, occaſioning a difficulty in the admiſſion of the charge on the outſide of the jar. And, in all caſes, the greater this difficulty of paſſage was made, provided the diſcharge was made at once, the more conſiderable was the lateral exploſion, and the greater ſhock was given to the hand that held the diſcharging rod; which ſhock was nothing more than one of theſe lateral exploſions, iſſuing from the rod as part of the circuit.

I ſhall conclude the account of theſe experiments with obſerving, that they may poſſibly be of ſome uſe in meaſuring the conducting power of different ſubſtances; ſince, the greater is the interruption in the electric circuit, occaſioned by the badneſs of its conducting power, the more conſiderable, *cæteris paribus*, is the lateral exploſion.

XIX. *Experi-*